Nomenclature and Common Sense

By A. A. Allen, B.Sc., A.R.C.S.

Our Editor calls for a readable article from some enlightened person entitled 'Nomenclature without Tears'. Far be it from me to attempt anything so difficult—or should I say impossible? Indeed, the subject becomes for many of us ever more lacrimose, whether the tears are of sheer bewilderment or mounting impatience and exasperation. But (though doubtless unenlightened) I am so much at one with the common-sense attitude both of Dr. Ainley (*antea*: 307) and of Mr. Jacobs in his appended note, as to be moved to defend it—at the same time venturing a few observations and personal reflections on points they have raised, with a suggestion or two as to the general line that might be adopted.

Dr. Ainley's title is a wryly apt comment on the state of affairs. As Mr. E. G. Bayford once remarked in a letter to me, it really is astonishing how easily an author (perhaps over-eager to see his name in print) can get a name-change adopted with little or no question, criticism or challenge. The fact seems to be that for a certain type of mind, nomenclature with all its ramifications and intricacies is liable to become an obsession and an end in itself, which of course can be disastrous. These enthusiasts rarely consider the hampering, distressing effect upon the progress of biological science that their policy, put into practice on an ever-growing scale, is bound to have; or when they do, they play it down to an extent verging on irresponsibility. Only thus is it possible to rate legalism above common sense in the naming of organisms. Now common sense demands a stable nomenclature, and that rules—useful and indeed indispensable as they are — must be so framed as not only to avoid interference with that stability but also to promote it actively; in fact, that is the whole point of having them. That they are only partly so framed (through a failure to grasp the essential requirements of a code) results in a great deal of needless confusion and difficulty, which in its turn tends to breed error; and the habit of upsetting and juggling with names has become a fashionable game amongst enthusiasts and often over-specialized professionals who have largely lost touch with the outlook and needs of ordinary entomologists.

The priority rule, whose uncritical application is at the root of so much of the trouble, is excellent as a *general* principle, *provided* however that it is subject to certain severe restrictions. Instead of that it has become, in many quarters, a sacred cow. It ought to have been foreseen that, if not so restricted, we should run into serious trouble through not having before us *at one and the same time* all the data necessary for arriving at the earliest name of many anciently founded species. The drafters of the International Code, however, seem to have been lawyers rather than working entomologists or zoologists, and to have taken it for granted (at least in the latest 1961 code) that it must be massive and complex and go into great detail—if so, a cardinal mistake. The artificial involutions of the law have little to do with the needs of a serviceable nomenclature. A consensus of rank-and-file entomologists is what we really want, not a ponderous bureaucratic set-up.

We who require above all that a name should be understood in one definite sense, and therefore fixed by usage rather than by edict, are often rebuked by the legalists as being selfish, lazy and reactionary, and as

putting short-term expediency and personal bias before ultimate scientific advancement; while they claim that their policy alone is conducive to stability in the long run. But is this quite fair? Readers may judge which position is the more realistic; and as for stability, facts speak for themselves! We are for ever promised 'jam to-morrow', but since the Commission was set up things have steadily become more chaotic. Even back in the '30s the controversy was raging in the *Record* and elsewhere—with, as it seems to me, the practical men winning easily on points. (For that matter, it has been with us for close on a century.) The telling facts and arguments brought forward by the late Professor Frank Balfour-Browne in numerous articles and notes are no doubt widely known, and I will not repeat them here, except just for his sensible proposal (rejected, of course, by 'officialdom') that "henceforth no change of a well-known name of a genus or species will be accepted if it is made solely by reason of the discovery of an earlier name" (1943(2)). In practice this would require slight extension, since simple priority is not the sole pretext on which names are needlessly altered, though it is the most common. In one point however I differ from Prof. Balfour-Browne and agree with the late Dr. G. W. Nicholson who wrote in 1932: "If Latin, however debased, is used at all in scientific language, it should, I submit, be used in accordance with the elementary and very simple rules of Latin grammar." This is also the view taken by the International Commission.

A suggestion I have seen made, that editors of journals should bring all names into conformity with the rules, is—apart from being distastefully authoritarian—quite impracticable because, even if they felt so disposed, few would have the necessary knowledge or time for the required checking. For the mere use of an up-to-date catalogue will not ensure even technical correctness. Compilers are very apt to follow blindly the latest authority; but different authorities (however eminent or learned) very often hold conflicting opinions, and who is to say which is right?

(Talking of conflicting opinions, our Editor tells us he has been taken to task by 'one of the authorities' for publishing an article on the present subject by Prof. Balfour-Browne. I am delighted that he is defiantly unrepentant! I hope most of our readers will agree that science is best served by the free expression, not the suppression, of differing views, and the discussion that flows therefrom. Unorthodox opinions may be shocking to some, but I would say: let those who disapprove, however exalted, demonstrate the superiority of their own if they can. It does no harm—indeed quite the reverse—to submit our most cherished assumptions to periodic examination in the light of reason.)

Many years ago the writer put forward a suggestion that a 'principle of longest use' might replace that of priority wherever an established name was threatened, so that the most-used or best-known name would be retained. The 'experts', naturally, poured scorn on the idea; but I have been gratified to see it gain ground steadily on the Continent—at least in the one Order, Coleoptera, where I can speak on this point. There many of the leading specialists and other prominent workers strongly support 'Kontinuitätsprinzip' (continuity principle), which amounts to the same thing. It seems to me that the best hope of sanity lies in this direction. In our country the movement has not been nearly so marked,

doubtless because Kloet and Hincks in their Check List, and other well-known authorities, have taken the opposite stand. This is doubly unfortunate in that there is consequent disharmony between the British and Continental catalogues as regards nomenclature. As Balfour-Browne has pointed out, many of the changes in the former are due to a stupid and arbitrary rule concerning homonyms, which our Continental colleagues for the most part wisely prefer to ignore; and it would be well if we were to come into line with them on these matters.

Things have now reached such a pass, with the rule-book swelling largely at each revision, that very many of us would favour cutting loose and starting afresh with a simple, clear and practical code of rules, free from lawyers' jargon, that could be put on a single page. At all costs we should avoid over-elaboration (making for cumbrousness) and not try to legislate for all contingencies, which is quite unnecessary. With the natural and inescapable complexity of his subject, and the output of literature increasing all the time, what working entomologist has the leisure to pore over 100 or more pages of tortuous legalistic phraseology, often so obscure that—like our law—it needs almost as much print again to make it intelligible to the average user? Meanwhile the non-expert will be well advised to adopt a cautiously conservative approach to name-changes and not to accept without question any that appear needless.

Before passing on to the distinct (though cognate) subject of generic splitting, I will just mention three further points. (1) A single change, insignificant perhaps in itself, may in turn set off a train of others if the rules are rigidly observed, thus generating a disproportionate upheaval. This fact should be quite sufficient justification for suppressing the original change. (2) The most obnoxious of all changes are transpositions or reversals, involving the switching over from one to another of two or more names of genera or species. Here it is not a matter of getting accustomed to unfamiliar names, but of the use of already familiar ones in reversed or new senses; with the strong probability of real error and confusion resulting. (3) The legalists are quite prepared to upset an established name if in the original publication it is preceded by another, held to refer to the same species, on an earlier page or even a few lines above. And that is not all. Suppose the prior of these relates not to the normal form of the species, but to a distinct variety: then this name, *even though properly that of a variety*, must yet be the valid name of the species itself. So we could have, say, a black insect named *niger*, and a yellow one *flavus* on the previous page which turns out to be a rare form of it. But the pundits will have it that the familiar name *niger* for the species must give way to *flavus*, notwithstanding that their 'new' *flavus* will then be a normally black insect! Such a *reductio ad absurdum*, perpetrated on the most trifling and flimsy of grounds, is by no means unknown and shows to what length fanaticism (or the want of a sense of proportion) will go. A very different matter from, say, 100 years' priority in one of the names! Pettifogging of this sort has helped to bring the Code into disrepute.

Both Dr. Ainley and Mr. Jacobs have made some good points about the creation of new genera out of long-established ones, and the rest of my remarks will mostly be devoted to that topic. 'New' genera in this sense, of course, result from the desire to give greater systematic

weight than hitherto to certain observed differences. No doubt some changes of this class reflect genuine advances in knowledge, and when it is seen that they are sound and necessary most of us learn to accept them with a good grace. Many more, however, are ill-conceived, thoughtlessly made, or quite unwanted; or at best highly questionable. There is certainly a very widespread present craze for indiscriminately multiplying genera by splitting up those long accepted in a given sense, and also (in some groups at least) for shifting species about from one genus (real or so-called) to another and sometimes back again. As Prof. Balfour-Browne remarks, "there seems to be a tendency in many of those who concentrate on smaller groups to raise the rank . . . . the number of genera with one species tending to make classification ridiculous." The Rev. E. J. Pearce writes: "I am sure that we have to guard against what seems to be a common tendency—the considerable multiplication of the number of genera, especially when they contain but one species." A vigorous and cogent plea for restraint in this practice was made by Dr. T. T. Macan in 1955, and supported in a shorter but important article by G. H. Hardy in 1956. The late Dr. K. G. Blair, whose experience and breadth of outlook command attention, had taken a similar line in a most interesting and thought-provoking paper on the correct name for the Dark Green Fritillary, to which I shall return. "Excessive subdivision of genera," he wrote, "is to be deplored as leading, especially in a limited fauna, to almost every species being placed in a genus of its own, and having therefore two names to be remembered; and thus defeats the whole object of binomial nomenclature, which is to assist us to retain a mental picture of the classification of the group."

Dr. Ainley and Mr. Jacobs may, I think, rest assured that they have behind them here a growing body of distinguished professional entomologists (to say nothing of the mass of amateurs). As the above writers stress, the innovations in question are very largely matters of opinion, different specialists having different ideas of what constitutes a generic character. Some, for instance, automatically give a generic rank to divisions and characters that others consider to be at most subgeneric. Although (as Blair remarks) the amateur student of a limited fauna has no basis for judgment here, it is clear that such innovations cannot in any way be regarded as absolute or final, and we should not feel bound by them. Soundly-based changes ultimately win general acceptance—the only external criterion of their soundness. Meanwhile, many too hasty or ill-conceived ones will 'fall by the wayside' after varying times.

Specialists who study the world fauna in their particular group, whilst alone in possession of the facts required for a decision in any instance, may yet err for the very reason that their specialism tends to give them an exaggerated view of the importance of the characters they seek to evaluate; and the more so, the more narrowly they specialise and thus very likely lose touch with broader issues and interests. It is surely most necessary in these matters to keep a sense of balance and perspective[1] *over the whole field*—or as much of it as possible. It is just

[1]It may be that the non-specialist alone can see when the balance gets badly upset. To take a concrete example, it surely is absurd that the two fritillaries *cydippe* and *aglaia* should be in separate 'genera' *while at*

here that discretion and caution are so often thrown to the winds in the first flush of classificatory zeal. Increasingly, characters are now widely used for founding genera, which 50 or 100 years ago would have been rated no higher than the specific level. Such a process cannot continue indefinitely if binomial nomenclature, with its great advantages, is not to break down—or (as Dr. Ainley says with much reason) become pointless.

So much for the existing situation; but what can be done about it? One might be tempted to begin by appealing to systematists everywhere never to make two or more genera where one exists already (above all if it has but few species) without having most earnestly asked themselves whether subgenera or species-groups would not do instead. The subgenus might well be made more use of, especially in the Lepidoptera where its availability seems to have been forgotten; but the same applies with even more force to the species-group, which is more elastic, more provisional, and very conveniently named from its most typical species. This in our state of near-ignorance offers immense advantages.

I fear, however, that such appeals would fall upon deaf ears; the hair-splitting habit has become too ingrained. As Mr. Hardy says, the taxonomist must first reform his own manner of thinking. Since there is no way of knowing whether a given innovation will stand the test of time, there appears to be only one sensible course for the amateur: instead of rushing to follow the nomenclature of the latest catalogue or up-to-date authority regardless of the policy adopted, let us hold our horses and stick to the names that everyone knows, at least until the neologisms have gained wide currency and there is no chance of their puzzling anyone. We shall then offend no one but the pundits, and shall merely be leaving the issues to more competent judges than ourselves.

This is the advice given in the three articles mentioned. Urging that some compromise must be sought, Dr. Macan writes: "Few wish to copy the antics of a kitten chasing its tail, which they will do if they try to keep right up to date by adopting every change the moment it comes out", and suggests ten years at least as a probationary period. He instances a species of *Corixa* that has had six different generic names within 25 years! This can only mean that the changes were premature and made more out of ignorance than knowledge, and the rest of us who want reasonable stability cannot be blamed for looking askance at the systematists' less responsible efforts. As Mr. Jacobs remarks, it is surely better to err on the side of too little splitting than too much, when there are obviously so many relevant factors still unknown.

Blair and Hardy both put forward the idea, which I strongly support, that during this protracted period of flux in nomenclature a 'double standard' should be permissible. Teachers, economic, agricultural, or medical entomologists, ecologists, general biologists, students of a limited fauna, popularisers and the great numbers of ordinary interested laymen, collectors, and naturalists—all alike have a vested interest in names which do not alter every few years, whether or not they are technically

the same time our *Papilio machaon* is made congeneric with the great 'bird-wings' of the Eastern tropics, which used to be in a genus *Ornithoptera* (seeming very natural) but now once more appear to be back in *Papilio*.

correct or officially sanctioned. These groups represent a broad spectrum, or a very considerable fringe on the periphery of the science, whose needs are by no means to be ignored; and those who affect to despise them lay themselves open to the charge of arrogance. The double standard (as Blair points out) is already employed to some extent by those who use subgenera. In a learned taxonomic paper, for instance, it is often more informative and convenient to use subgeneric names in place of generic where all students of the group will know what genus is under discussion, whereas in one of wider scope the generic names will naturally be used. In a similar way it should be legitimate in general contexts, and for all purposes that might be classed as exoteric rather than esoteric—and without any imputation of error or ignorance arising —to use certain generic names in a more extended and 'popular' sense than the strict purist or specialist might approve. They would at least then be intelligible to *all* concerned. Any doubts could be assuaged by the insertion of 's.l.' (for *sensu lato*, 'in a wide, or the wider, sense') in brackets after the name of the genus, thus in effect invoking against possible objectors the whole weight of traditional usage.

Dr. Ainley's reference to the Fritillaries affords a good example. By all means let us continue to call those he lists by the familiar and still much used generic name of *Argynnis* (except, perhaps, when we happen to be concerned with the larger fauna of Europe — in which the subdivisions have more point—or venture into the more difficult and disputed territory of the species' relationships, meeting the specialists on their own ground). For catalogue use and labelling the collection, I suggest inserting the newer 'generic' names (*Fabriciana, Clossiana*[2], etc.) *as subgeneric*, by way of compromise; and similarly in parallel cases. Very probably they rate no higher anyway, and in time even the systematists may revert to a wider conception of *Argynnis* as before (and as in Kloet & Hincks as late as 1945). The criteria for the separate 'genera' can hardly be other than slight; for one author, Francis Hemming, who used them all in 1942, was definitely of the opposite opinion in 1934.

I cannot, alas, tell Dr. Ainley what those criteria are. However, suspecting that some of them may be sexual, I would take the opportunity to urge caution in the erecting of genera on characters present in one sex only, as I think is increasingly done. (Their possible *confirmatory* use is, of course, another matter). There is surely something unsatisfactory about a criterion present in only half the members of a population. True, we cannot avoid it at the species level, and even there it is bad enough when one sex (usually female) is not determinable by inspection or even dissection. In any case, the sexual characters—both primary ('external' genitalia) and secondary—give in many groups an impression of being too recently acquired to indicate relationships more fundamental than those between species themselves, when we consider the remark-

[2]I wonder whether the substitution of this for the far more familiar *Brenthis*, which held the field for a very long time, was really necessary. I notice too that some recent lists have been using *Mellicta* instead of *Melitaea* for *athalia* Rott.; but is it worth more than a subgenus?

[I leave this note as originally written, though in fact Mr. Warren in the February *Record* clarifies the point as regards *Clossiana*, etc., besides certain others I have touched on.—A.A.A., 1.iii.1968.]

ably wide differences they can assume among species otherwise scarcely distinguishable.

And now a piece of good news for Dr. Ainley and Mr. Jacobs. Their bugbear, *Mesoacidalia charlotta*, can, I rejoice to say, be exorcized and forgotten at least by the butterfly-lover unconcerned with taxonomic niceties, and who is content with the excellent advice offered by Dr. Blair (himself, be it noted, a professional systematist). Incidentally, I should dearly like to know what possessed Herr Reuss in 1926 to light upon the monstrosity *Mesoacidalia* for a genus of fritillaries (of all things!). I trust that no enthusiast will come up with a name like *Protoargynnis* for some of the 'waves', but if he did it would be no more ridiculous. Blair questions whether this case of generic splitting will be widely accepted, and clearly favours for it a lower status, but either way I do not think we need worry. As for the specific name, he shows conclusively that Hemming, who had earlier accepted the Linnaean *aglaia* or *aglaja* (1758), misinterpreted the Rules in trying to establish in its place Haworth's *charlotta* (1803); and that in any case *charlotta* Haw. is neither the first described form nor the ordinary British one, but a well-known though scarce variety. (The rule that requires a species to take the name of its first-described *variety*, if no earlier name is valid, seems to have dubious consequences and should, I think, be scrapped).

I am not quite happy about Dr. Ainley's suggestion of an editorial strait-jacket for streamlining nomenclature in the Lepidoptera, even though made in an excellent cause. Regimentation of any kind is incompatible with the scientific spirit, and I feel that individual writers must still be free to choose, where so much remains a matter of opinion. As regards the butterflies, one might do far worse than follow Kloet & Hincks—a good list—as Dr. Ainley recommends; and I note that the new edition of the Lepidoptera part, on which great hopes are pinned, is due this year. (The moths, on whose systematics so much work has been done since the first edition in 1945 — and especially, of course, the 'Micros'—pose more of a problem). There will naturally be certain cases, when we are following the policy of the most-used name, where it will be hard to decide which that is; but then it will not greatly matter which is selected. Dr. Ainley mentions *Lysandra bellargus* and *Lycaena adonis*, to which one could add a third variant: *Agriades thetis*. The first now seems to have won the day, at least here.

Although the changes made by the priority-hunters and their kind are in a different class from those of the splitters, their cumulative effect is similar since they reinforce each other in undermining stability. As they involve names of species as well as genera and do not even pretend to any practical utility, or to reflect new knowledge, it follows that they are still more objectionable in the mass. I suggest that our attitude to them—or those of them which are not clearly and thoroughly justified—should be the same: a healthy scepticism, reserving the right to follow a more rational path. On one point even we non-experts may and should insist: that full and sufficient grounds be given for any change made or proposed, no matter how impressive the authority (cf. my footnote on *Augiades* versus *Ochlodes*, *Ent. Rec.* **79**: 61). There must, I think, be some such curb upon abuses of the freedom to alter names once accepted. One consequence will be that changes made in catalogues or

lists, unless justified in print elsewhere, shall not be binding at least until their adoption is almost universal both here and abroad.

It is now evident that some of the alterations that have crept into our lists from time to time were needless from any point of view, being based on an over-zealous, excessively narrow, or downright faulty interpretation of the Code. It is only fair to add that the International Commission has increasingly of late years conceded the principle that changes liable to cause serious confusion should not be made. The pity is that this recognition has come so late (I had almost said *too* late), after so much harm has already been done through the spread of the habit of playing fast and loose with our nomenclature. The hopes must be for a change of heart—a larger vision, a less finicking and more generous attitude, with the resolve to profit from past mistakes and rebuild on a more realistic foundation. I see little chance, however, of that happening without collective and effective pressure by all interested parties; meanwhile, perhaps, rising discontent will force a crisis, which may bring the recovery of sanity nearer.

REFERENCES

(Of the great many relevant papers and articles, only the few mentioned above can be listed here.)

Blair, K. G. 1943. *Argynnis aglaja* L. v. *Mesoacidalia charlotta charlotta* Haw. (Lep., Nymphalidae). *Ent. mon. Mag.*, **79** : 78-81.

Browne, F. Balfour. 1943 (1). What shall we call the Dark Green Fritillary : *Argynnis aglaja* (L.) or *Mesoacidalia charlotta charlotta* (Haw.) (Lep., Nymphalidae). *Ibid.* : 46.

———. 1943 (2). Limitations on the use of the Rule of Priority. *Ibid.* : 108.

Hardy, G. H. 1956. On new generic names, their acceptance or otherwise. *Ibid.*, **92** : 299-300.

Macan, T. T. 1955. A plea for restraint in the adoption of new generic names. *Ibid.*, **91** : 279-282.

GEOLOGY AS AN ECOLOGICAL FACTOR IN THE DISTRIBUTION OF INSECTS

By Alan E. Stubbs

(*concluded from p.* 59)

DISCUSSION ON DISTRIBUTION

Now that the available information on the ecology and possible ecological requirements of *S. immaculata* have been considered some general discussion on distribution may be useful.

We find that grassland, often with associated scrub, is the preferred habitat—one of the most widespread vegetation types and occurring on a wide range of soils. Within this vegetation type, one must apparently narrow the most suitable terrain to those grassland localities occurring on chalk or limestone. However, the outcrops of chalk and limestone in southern and eastern Britain are widespread and extensive, even allowing for the large areas of these outcrops covered by non-calcareous superficial deposits. Yet, surprisingly, *S. immaculata* is a rare species, very local and often scarce when found, though locally plentiful.

The species used to be regarded as very rare, in fact the majority of dipterists would still consider it rare, yet there is a noticeably steady increase in records since the war, and especially for recent years. It is, of course impossible to get over the problem of bias caused by consulting